After a somewhat diffuse essay on the hearth of the ancients—in which, by the way, the theory is too easily adopted that the Romans were entirely ignorant of the chimney—we come to the methods of our ancestors. Why the cosy folding screen must henceforth be given up, and why, in speaking of the introduction of the smoke-jack, the past services of the turnspit dog are entirely ignored, may be asked in passing.

Page 17 introduces some interesting particulars of the first use of coal, and page 30 a well-merited eulogium on the labours of Count Rumford in the improvement of fire-grates, and we now approach the pith of the whole matter, namely—how to burn our coal so as to get the maximum of heat with the minimum of smoke and soot. (pp. 44 *et. seq.*) Here the author, having one excellent contrivance to recommend (namely, Dr. Arnott's smoke-consuming grate), has allowed himself to be unfair to other inventors whose grates for certain purposes are preferable. Of this class perhaps the most conspicuous example is the manner in which Taylor's stove, manufactured by the Coalbrookdale Co., is treated in the work before us. It is indeed mentioned (page 51) as an ingenious contrivance but full of deficiencies in practice, and as one which "now appears to have become forgotten." As respects its deficiencies, several instances are well known to us in which this grate of Mr. Taylor's has given the greatest satisfaction. No unsightly contrivances—no troublesome machinery—a bright fire—the whole apartment pervaded by the fresh warm air brought from an external source to the back of the stove, and emitted from the radiating tiles—the chimney-sweeper's visits for years dispensed with—such are some of its characteristics; and as to its "having become forgotten," assurance has been received from good authority that the demand for these grates is well kept up.

There are also several stoves which in a work like the present should have been noticed, *e.g.* Wright's, Woodcock's, Rosser's, and especially Galton's.

At page 54 the author discusses the recommendations of the committee appointed by the Board of Health, and gives some valuable comments upon them, and then proceeds to consider the question of the *Stove of the Future*, in its materials and management. Nothing has been more clearly established than the superiority of fire-brick over iron for the "fire-pot," or actual inclosure of the fuel. But the benefit of this discovery has, as yet, only reached the upper and middle classes of society, "but," says the author, p. 66, "in small houses and in the apartments occupied by skilled and unskilled workmen, the use of fire-brick is unknown, so that those who most require to use economical grates are those who are most ill provided. The working classes have either the most trumpery contrivances that Thames-street can furnish, and which are put into the fire-place at the smallest possible expense for labour and materials; or they use, in streets which have become gradually deserted by the classes which once lived in them, the badly arranged fire-places of thirty or fifty years ago."

We hope that these valuable observations will not be lost. Such fire-places as Pierce's Cottage Grate, described p. 39, are scarcely more expensive than the cheap and trumpery contrivances referred to, and if the proper arrangement of the chimney throat could also be secured in all newly constructed houses for the working-classes, the effect would be soon apparent both in favour of their health and of their pockets.

This improvement of the chimney throat is described by the author p. 73, and consists in contracting the flue to its ordinary capacity at once at the top of the fire-place opening, instead of the more common practice of gathering it in by degrees.

In p. 85, the plan of having a single main chimney-shaft for all fire-places which are situated in the same part of the house is advocated. It is not evident, from the text, whether the author is aware that this plan has been carried out, in several instances, in different parts of the country. It is a subject, certainly, which deserves more attention than it has yet received.

In the last chapter (p. 90), the general warming of halls, corridors, &c., is considered. It may be mentioned in connection with the Russian and Swedish method referred to, that the Germans, who have hitherto adopted generally the same plan, are beginning to place the fire-front of the stove in the apartment that is to be warmed by it, instead of in the corridor, with the stove surface only in the apartment, and, as may be supposed, with a manifest improvement in the ventilation.

This chapter is well worth attention: exception must, however, be taken to the way in which Gurney's stove is mentioned, p. 108. "The prestige attaching to the name of its producer," may, indeed, have not been without its use in obtaining for these stoves their first trial, but would hardly have been sufficient, apart from their intrinsic good qualities, to have obtained for them the wide reception both at home and abroad which they have had; or to have created the almost invariable satisfaction with which their use has been attended.

The book is a useful contribution to the literature of the subject, and well illustrated by engravings. F.C.P.

OUR BOOK SHELF

The Mammalia of Massachusetts. By J. A. Allen. (Trubner & Co.)

FROM this carefully drawn-up report we learn that there are sixty-five mammals at present indigenous to this American State. The common ones, with a few exceptions—as the mink (*Putorius lutreolus*), weazel (*P. ermineus*), and skunk (*Meflutis mephitica*), among the carnivores; *Vespertilio subulatus* and *Lasiurus noveboracensis* among the bats—belong to the families of the rodents, the squirrels, mice, and hares, and to the *Balænidæ* and *Delphinidæ*, which latter are of course marine. The panther, moose, reindeer, elk, and beaver have comparatively recently become extinct. A notice is appended to the work by Prof. Agassiz, earnestly requesting friends to forward to him males, females, and young of almost any of the European mammals. Books on mammalia would be equally valued, and in exchange he offers on the part of the museum at Cambridge, U.S., representatives of the North American fauna and American books.

Effects of Climate and Soil upon Plants.—*Die Abhängigkeit der Pflanzengestalt von Klima und Boden.* Von A. Kerner. Pp. 48. (Innsbruck, 1869. London: Williams and Norgate.)

THIS pamphlet is of greater importance than would be indicated by its mere size, as a contribution towards an investigation of the causes which lead to the diversities of floras, and hence towards a knowledge of the laws on which depends the great problem of the origin of species. M. Kerner has made a special study of the flora of the

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Tyrolese Alps, and has even attempted to introduce into a small plot of ground in the mountains surrounding Innsbruck, a number of plants indigenous to the lowlands of the Tyrol. In this enterprise, however, he met with no very encouraging success; "the greater number of the plants which I brought to those heights with inexpressible toil, succumbed to the uncongenial Alpine climate; and in the remaining small portion, I have noticed at present only very unimportant changes." His conclusion from these experiments is "that changed conditions of life can kill the species, or they can reduce it to a starved existence, but can in no case produce a *direct* change into a new permanent species adapted to its altered conditions." Such change can only take place by the slow process of natural selection among slightly varying offspring from the parent species. The writer notices a number of interesting features that characterise the Alpine flora with which he is familiar, as contrasted with those found under other climatal conditions. One of these is the very small number of annual plants, which bear to perennials the proportion of 4 to 96, as contrasted with that of 42 to 58 in the Mediterranean district, and of 56 to 44 in that of south-eastern Europe; a result of the very short period of summer warmth, varying from 1½ to 3½ months, which does not allow time for the seeds to ripen. The same cause produces also the appearance in many Alpine species of the flower-buds at the close of the summer, ready to burst into blossom during the first days of returning warmth in the spring. The remarkably large proportion of Alpine plants with evergreen rosettes of fleshy or succulent leaves, *Primulas*, *Gentianas*, *Androsaces*, *Saxifragas*, *Drabas*, &c., he attributes to the advantages of some contrivance for obviating the effects of the intense heat of the sun during the long days in their short summers, and also to the necessity that the plant should possess leaves at the very commencement of the warm season, in order to afford it a store of nourishment, and thus economise the whole of the brief period of vegetation. With this peculiarity he contrasts the poverty of the Alpine flora in plants possessing stores of *underground* nourishment in the form of bulbs, a class so abundant and prominent in the south of Europe. The necessity for great caution in deriving general conclusions from a small array of facts, is shown by the mention by M. Kerner, among the plants well adapted by their constitution to withstand the great alternations of an Alpine climate, of *Dryas octopetala*, a species which flourishes equally well in the remarkably uniform climate of the west coasts of Ireland and Scotland. The want of any considerable number of large shrubs and forest trees is obviously due to the rigours of the climate; and the almost entire absence of climbing and creeping plants indicates that protection from the sun is not one of the first conditions of existence, as it is in tropical forests. The large proportion of plants with flowers of intense hues, and the deficiency of spiny and stinging species, are not so easy to account for, though the author attributes the latter to the comparative absence of destructive animals; and the former may possibly have some connection with the advantage derived from the speedy attraction of insects, after the flowers expand, to assist in their fertilisation. We can conceive no greater service to biological science than a series of observations on the floras of limited areas, both with respect to what they possess and to what they are deficient in, carried out with the care of those recorded in the work before us.

A. W. B.

The fourth volume of the *Atti della R. Accademia delle Scienze di Torino*, contains several important papers on various departments of science. We may notice especially Prof. Salvadori's memoir on some birds from Costa Rica, and the same author's monograph of the genus *Ceyx*; a memoir by M. F. Giordano, on the orography and geological constitution of the Gran Cervino; and mineralogical papers by Prof. Strüver and Dr. Cossa.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

Evidence concerning Heterogeny

THE question of the truth or falsity of "heterogeny," as it is called, is perpetually recurring both in your columns * and elsewhere, in connection with several of the most important scientific controversies of the day. It is a subject which has engaged my attention for several years, and I am anxious to be permitted to lay before your readers as concisely as possible a statement of what appears to me to be its present position.

I think the impression which most recent references to it are likely to leave on the minds of readers is this—viz., that while amongst the most advanced thinkers there is a gradually strengthening conviction that the weight of theoretical considerations is in favour of the actual existence of heterogeny as a real mode of origin of living beings, yet that the authority of M. Pasteur's famous researches inclines the balance of experimental evidence heavily the other way.

I am perfectly willing to admit the principle of authority in matters scientific as far as any reasonable person can admit it—that is to say, I believe it is natural and right that when a scientific man of so deservedly high reputation as M. Pasteur publishes a long series of carefully conducted researches, and announces the conclusions to which they lead him, and when no evident flaw can be shown in his processes, either of experimentation or of reasoning, his conclusions should be accepted as against those of another comparatively unknown experimenter. But in the present case neither of these conditions is fulfilled. In the first place, instead of merely an unknown experimenter we have in this case a consensus of at least eight experimenters—some of them by no means unknown—who have given their best attention to the same investigation in different parts of the world and under widely differing conditions, and who agree in disputing M. Pasteur's results.†

In the next place, there is a step in M. Pasteur's experimentation which has been pointed out as a flaw, and of which it is in the power of any one of your readers to judge for himself by a process of simple inspection as to whether it really be a flaw or not. I refer to the subject of the microscopic power used by M. Pasteur in his investigation. The organisms which I found during my own experiments on this subject appeared often but little larger than a full point in the type used in your columns, even when seen with a power of 1700 diameters. In the woodcuts attached to my paper in the Proceedings of the Royal Society (April, 1865), they may be seen as they were kindly drawn for me by my friend Dr. Beale, who, though an uncompromising opponent of the doctrine of heterogeny in all its forms, is a very high authority in microscopy; there can therefore be no question as to their size, nor as to their actual presence in the experimental vessels. Now under the power used by M. Pasteur (350 diam.) these organisms would be about $\frac{1}{25}$ part of the size which they appear in the drawings. Yet it is upon the authority of observations made with such a power that M. Pasteur has pronounced, not upon the *presence* of these objects, but upon their *absence*. It is now nearly five years since my observations were made public. Since that time several critics have noted them as requiring an answer, but, so far as I am aware, no answer has been made to them; and meanwhile naturalists have gone on complacently quoting M. Pasteur's experiments as having settled the question against heterogeny, even though they have not failed to acknowledge the weight of the theoretical considerations which tell in its favour.

The theoretical aspect of the question I have fully discussed elsewhere,‡ and I will only here state my entire agreement with the belief expressed by Dr. Charlton Bastian in your issue of February 24, viz. "that the time is not far distant when the doctrine of the evolution of living things will be as much an

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* NATURE, Feb. 1, p. 351; Feb. 24, p. 424.

† 1. Pouchet, "Nouvelles Experiences" passim. 2. and 3. Joly and Musset, *Comptes Rendus*, 1860. 4. Schaafhausen, of Bonn, *Comptes Rendus*, 1860 and 1862. 5. Mantegazza, of Pavia, see *Cosmos*, 1863, p. 630. 6. Wyman, Harvard, U.S., *American Journal of Arts, &c.*, vol. xxxiv., July 1862, vol. xliv., September 1867. 7. Proceedings of Royal Society G. W. Child, Oxford, June 1864 and April 1865. 8. Hughes Bennett, Edinburgh, *Edinburgh Medical Journal*, March 1868.

‡ "Essays on Physiological Subjects," 2nd edition, pp. 137—154.

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